THE ANALYST.

MARCH, 1893.

PROCEEDINGS OF THE SOCIETY OF PUBLIC ANALYSTS.

A Special Meeting of the Society of Public Analysts was held in the rooms of the Chemical Society, Burlington House, on February 15th, 1893. The meeting was convened to consider a series of resolutions drafted by the Council on the subject of certain proposed amendments in the laws relating to adulteration. There was a large attendance of Members of the Society, and of other gentlemen interested in the working of The Food and Drugs Acts. In the unavoidable absence of the President (Sir Charles Cameron), the chair was taken by Mr. M. A. Adams. Amongst those present were Dr. Cameron, M.P., The Hon. H. A. de Tatton Egerton, M.P., Mr. Kearley, M.P., Mr. G. M. Allender, &c. Letters of regret at inability to be present were read from Sir C. Cameron, President of the Society, Sir Walter Foster, M.P., Mr. Brunner, M.P., Col. Howard Vincent, M.P., Mr. H. J. Wilson, M.P., Sir Henry Thompson, Sir W. Pink, Sir Joseph Fayrer, and many others, including most of such provincial Public Analysts as were unable to be present. The following resolutions were put to the meeting and carried unanimously:—

1. That amendment of the laws relating to adulteration is urgently required.
2. That the present Acts often operate unfairly on the retail traders, and that provision is necessary to ensure in many cases of adulteration the prosecution of the real offenders.
3. That in view of the fact that, as is shown in the Local Government Board reports, the Food Acts are practically dead letters in a large area of the United Kingdom, it is necessary that adequate provision be made for securing uniformity in their application and in their due enforcement.
4. That in order that the Local Government Board should have better control over the working of the Acts, a portion of the expenses of working them should be borne by the Imperial Revenue.
5. That in view of this, it is desirable that there should be a duly constituted Chemical Department of the Local Government Board with whom the Public Analysts, as officers of the Local Government Board, should be placed in direct relation.

6. That the present system of reference in the case of disputed analyses is unsatisfactory, and ought to be entirely remodelled.

7. That the compulsory combinations of the two offices of Medical Officer and Public Analyst is in the public interest undesirable.

8. That provision should be made to ensure better than heretofore the proper qualification of officers under the Act.

A full report of the meeting will appear in our next issue.

The usual Monthly Meeting of the Society was held on Wednesday, 1st February. In the absence of the President, on the motion of Mr. Hehner, seconded by Mr. John Hughes, the chair was taken by Mr. Alfred H. Allen.

The minutes of the last meeting were read and confirmed.

The following gentlemen were proposed as members:—Mr. Thomas Richard Duggan, F.I.C., F.C.S., Sunny Bank, Vanbrugh Hill, Blackheath, S.E.; Mr. W. H. Symons, F.I.C., F.R.M.S., F.C.S., 130, Fellows Road, South Hampstead. Mr. W. S. Crocker, assistant to Mr. J. Brierley, and Mr. James Sykes, assistant to Mr. G. Jarmain, were proposed as associates.

Messrs. W. Cobden Samuel, M. J. Sheridan, B. T. Thomson, and John White, were duly elected members of the Society.

Mr. Richmond then read the following paper: –

THE COMPOSITION OF MILK AND MILK-PRODUCTS.

By H. Droop Richmond.

This paper is a continuation of the annual reports of Dr. Vieth on his work in the laboratory of the Aylesbury Dairy Company. The work done in 1892 is treated of in the present communication. (For previous reports, see Analyst, vii. 53; viii. 33; ix. 56; x. 67; xi. 66; xii. 39; xiii. 46; xiv. 69; xv. 44; xvi. 61; and xvii. 62).

The total number of samples analysed in 1892 was 25,931, viz.:—

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23,865	samples	of milk.
1,338	,,	cream.
566	,,	skim-milk.
8	,,	butter-milk.
78	,,	butter.
24	,,	water.
22	,,	sundries.

Of the milk samples 13,196 were taken from the delivery churns on arrival of the milk from the country. The bulk is distributed with the least possible delay to the

customers, portions being, however, kept for the production of cream. For the control over the men employed in delivering the milk, a further 9,103 samples were taken before, during, and after delivery, and analysed comparatively. The following table contains the result of these analyses :—

	On Arrival.				Before Delivery.	At Commencement of Delivery.	During Delivery.	After Delivery
	Spec. Grav.	Tot. Sol.	Fat.	S.-N.-F.	T. S.	T. S.	T. S.	T. S.
January ...	1·0322	12·91	4·02	8·89	12·77	—	12·79	12·80
February ...	1·0323	12·84	3·95	8·89	12·72	—	12·75	12·83
March ...	1·0322	12·74	3·88	8·86	12·72	—	12·73	12·72
April ...	1·0322	12·54	3·72	8·82	12·46	12·31	12·44	12·49
May ...	1·0323	12·54	3 70	8·84	12·49	12·41	12·42	12·50
June ...	1·0322	12·42	3·62	8·80	12·32	12·25	12·24	12·40
July ...	1·0316	12·47	3·74	8·73	12·39	12·33	12·34	12·44
August ...	1·0315	12·57	3·89	8·68	12·48	12.41	12·45	12·48
September	1·0316	12·71	4·00	8·71	12·67	12.56	12·67	12·68
October ...	1·0318	12·94	4·15	8·79	12·81	12·72	12·73	12·81
November	1·0318	13·03	4·23	8·80	12·79	12·76	12·78	12·85
December...	1·0318	12·80	4·02	8·78	12·61	12·63	12·76	12·73
Average	1·0320	12·71	3·91	8·80	12·60	(12·54)	12·59	12·65

During the first three months of the year the quality of the milk was fully up to the average, but in the later months a depression, especially in solids-not-fat, was noticed. This causes the yearly average to be the lowest yet observed, lower even than last year. A remarkable instance of low solids-not-fat has already been brought under the notice of the Society (ANALYST, xviii., 4.) In this case I have shown that the low solids-not-fat was due to the milk sugar being below the normal quantity; and I may take this opportunity of expressing my opinion that it is important in cases of abnormal milk to make as full an analysis as possible, as notable differences from the normal proportion of some particular constituent may be detected, and the sample thus deprived of its value to the unscrupulous chemist who defends adulteration cases on the principle expressed in the proverb "se non è vero, è ben trovato." The differences between the five series of analyses is rather more marked this year than in previous ones. An explanation of these differences is to be found in a paper by Dr. Vieth (ANALYST, xvii., 86). As usual, the highest percentages of total solids and fat are to be found in November, while, as is frequently the case, the lowest occur in June.

Cream samples were taken for analysis before and during delivery. The average of the results is given in the following table :—

AVERAGE AMOUNT OF FAT IN CREAM.

	Before Delivery.	During Delivery.
January	46·3 ..	46·0
February	47·1 ..	47·5
March	47·0 ..	47·3
April	48·3 ..	48·9
May	48·3 ..	48·9
June	45·9 ..	46·5
July	48·5 ..	49·5
August	46·0 ..	47·3
September	46·6 ..	47·4
October	47·1 ..	47·1
November	44·3 ..	43·9
December	44·2 ..	44·1
Average ..	46·7	47·0

The average composition of Clotted Cream was as follows:—

Water	35·63 per cent.
Fat	56·27 „
Proteids and Milk Sugar ..	7·52
Ash	·58

Skim Milk produced by centrifugal cream separators contained, as a rule, from ·2 to ·4 per cent. of fat.

Butter had the following composition:— See 1891 p. 3.

French Butter, fresh:—

Fat	85·49 to 83·12.	Average	84·39
Water	14·93 „ 13·29.	„	13·98
Solids-not-fat ..	2·22 „ 1·09.	„	1·63
Including salt ..	·18 „ ·07.	„	·12
Reichert-Wollny Fig.	30·8 „ 25·4	„	29·1

French Butter, salt:—

Fat	85·35 to 81 93.	Average	83·44
Water	14·32 „ 11·29.	„	12·86
Solids-not-fat ..	4·29 „ 2·73.	„	3·70
Including salt ..	3·08 „ 1·26.	„	2·07
Reichert-Wollny ..	32·8 „ 26·2	„	29·1

English Butter, salt:—

Fat	... 86·19 to 80·14.	Average	82·98
Water ...	... 16·49 „ 11·58.	„	13·99
Solids-not-fat	... 4·98 „ 1·78.	„	3·03
Including salt	... 3·93 „ 1·16.	„	2·14
Reichert-Wollny	... 30·8 „ 24·9	„	28·1

A few odd samples of Danish and New Zealand butter were analysed; the Reichert-Wollny figure of the latter were high, being in three samples 32·1, 31·7, and 32·8. These figures are not remarkable, but they show that the large amount of volatile fatty acids in antipodeal butter, which has been remarked by several observers, seems to be normal. Nothing abnormal has been noticed this year; but the search for these samples has not been so thorough as in previous years. The samples giving the lowest Reichert-Wollny figures were prepared from the milk before referred to as being abnormally low in solids-not-fat. Two samples of this butter gave 24·9 c.c. of N/10 alkali consumed.

An opportunity occurred of studying the changes that occur on freezing milk. The samples under notice are remarkable, as the ice and the liquid portion show a greater difference than has hitherto been observed. Their composition is as follows:—

	Ice.	Liquid.
Water	96·23	85·62
Fat	1·23	4·73
Sugar	1·42	4·95
Proteids	·91	3·90
Ash	·21	·80
Specific gravity	1·0090	1·0345

The quantity of ice amounted to about 10 per cent. in this case; the proportions that the various constituents bear to each other is not markedly different in the ice and the portion which is unfrozen, showing that no great separation, if any at all, has taken place during freezing.

A number of comparisons have been made during the past year between the Adams and Werner-Schmid methods of fat-estimation; the result has been practically absolute agreement. As the Werner-Schmid method, as worked in the laboratory under my charge, differs in some respects from the usually published methods, I think it of interest to give a short description; it is exactly the same modification that I worked three years ago in Egypt, and consists in taking 5 c.c. of milk in a stoppered tube (I now use a Horsley's tube) and diluting with 5 c.c. of water; about 11 c.c. of strong hydrochloric acid are added, and the whole boiled over a naked flame till the fat forms a clear layer on the top, the tube being constantly shaken during the boiling. Immediately the boiling is completed the tube is cooled. About 25 c.c. of ether are then added and the whole well shaken and allowed to settle. The separation of the two layers is practically instantaneous, and no fluffy-looking layer appears at the junction of the ethereal and aqueous strata; as

much as possible of the ether is pipetted off, and a further quantity is added, and the treatment repeated three times over; the ether is evaporated and the fat weighed. This method, for the introduction of which into this country chemists owe a debt of gratitude to Mr. A. W. Stokes, has the advantage over the Adams method that the fat can be estimated within an hour or so of the receipt of the sample, in those rare cases where this is necessary; in this, however, it stands inferior to the Leffmann-Beam process. For convenience it is, in my opinion, markedly inferior to the Adams method, and involves more labour where many samples are done. This method has been claimed to be more theoretically perfect than the Adams process by Mr. Stokes (ANALYST, xvi., 71); as the Adams method is after all our official standard process, I may be pardoned for pointing out the fallacy of the reasoning of this chemist. He states that the most perfect medium for extraction is a gas, the next a liquid, and last a solid stands in order. The conditions that decide the suitability of a medium for the purpose of fat-extraction are—penetrability, insolubility of extraneous substances, and immiscibility; obviously, if the solvent and the medium are in two different states their immiscibility is better ensured than in the contrary case; two liquids possess, perhaps, slightly more penetrability than a liquid and a solid, but in the case of any reliable method of fat estimation from solids, this advantage is practically nil; the solubility of extraneous substances is decidedly greater in a case such as the Werner-Schmid method; the advantage rests on theoretical grounds with the Adams and similar methods; the extraction from a gas is obviously much more difficult than from either of the other states of matter, and it surprises me that such a statement should have been allowed to pass unchallenged. In showing the fallacy of Mr. Stokes' reasoning, I do not wish to unduly disparage the Werner-Schmid method, but my intention is to contest the assertion that the Adams method is inferior. The Werner-Schmid method gives very good results indeed, but it is scarcely so reliable as the Adams process.

The estimation of total solids has been much studied during the past year; a slight modification of Babcock's method has already been described (ANALYST, xvii., 225); this method has been used to a considerable extent. The results by this method have been a little higher than by the ordinary process; the actual proof that this is more correct than the usual process is not yet complete, but the following indications of this may be adduced: 1. The agreement between the fat found and that calculated by the "milk-scale" is relatively constant, a variation of 0·2 per cent. covering the whole of the experiments. 2. Complete analyses, in which water, fat, milk-sugar, proteids, and ash are estimated, add up to about 99·8 per cent. One would naturally expect an analysis comprising the constituents enumerated to add up a little below 100 per cent., on account of the presence of traces of lactic acid, urea and other bases, salts in the milk not included in the ash, &c., and from our present knowledge of the composition of milk, 0·2 per cent. is a very reasonable approximation of the quantity of these unestimated bodies.

A very extensive investigation, comprising 2,930 analyses, was made regarding the

rising of cream in the churns in which milk is taken out for delivery; it would be impossible to give here the results of all the analyses made, but the averages are to be found in the first table in this paper. The general conclusion was that the tendency of cream to rise was overcome by the constant slight agitation in transit, provided that at no time the rising of the cream was allowed to commence by a prolonged standing; if this had been allowed to take place the shaking was not sufficient, and the cream steadily rose; even in rounds which were out for six hours no rising of cream could be detected, provided that no long intervals of time were permitted in which the milk remained at rest.

A series of comparative experiments was carried out on the relations between the Reichert-Wollny and Leffmann-Beam methods of butter analysis. The relations were:— Reichert-Wollny : Leffmann-Beam = 2·20 : 1 (ANALYST, xvi., 153). In the Leffmann-Beam method only 2·5 grammes were taken and 50 c.c. distilled out of 75 c.c. The ratio between these proportions, alcohol being used in both, has already been found to be 2·23 : 1 (ANALYST, xviii., 17). This shows that the glycerol saponification gives slightly higher results than the alcohol saponification, and this has been confirmed by direct experiment. The use of glycerol has several distinct advantages, among which may be enumerated sharper end reaction, clear distillate, absence of possibility of loss of ethers during saponification, and saving of time.

DISCUSSION.

Mr. Allen said that when it was considered how small a part of his work Mr. Richmond had shown in the figures which he had placed on the board, it would be easily seen how extremely indebted the members of the Society must be to him for laying before them the annual average results of all the samples of milk analysed by him. Mr. Richmond's experience of the Leffmann-Beam process of estimating volatile fatty acids, as distinguished from the Reichert process, was of much value. He would remind the members that the Leffmann-Beam method consisted in saponifying with a solution of alkali in glycerin, by which means the tendency to the formation of butyric ether was avoided. He (Mr. Allen) had used the Leffmann-Beam process, and he found it gave results somewhat *higher* than the Reichert. It was, no doubt, a question of manipulation, for the American Association of Official Chemists reported exactly the contrary, namely, that the Leffmann-Beam results were lower rather than higher. He was glad to see that the figures obtained by the Reichert-Wollny process fell within reasonable limits. He was also interested in noting that the milk yielded by antipodean cows contained a higher proportion of fatty acids than was the case on this side of the globe.

With regard to Dr. Adams' coil process of determining fat in milk and the Werner-Schmid process, he had of late been using the Werner-Schmid process, because it was less troublesome, and it had been proved that the two processes gave substantially identical results. If two processes gave the same results, he did not see why the simpler one should not be used. The coil method was the official method of the Society; but it was

possible that evidence might be adduced in the future which might induce analysts to abandon it, or replace it by another process; and he could not endorse the view which Mr. Richmond apparently held that an analyst who used any other than the official process was a sort of traitor to the Society.

The total proportion of water found in butter was of interest as showing that the opinions held by public analysts, who had considered the matter, were well borne out; the only instance where it came over 16% being in an experimental sample. He had very strongly laid down in the witness-box and elsewhere that 15% was the maximum quantity of water to be allowed in butter, which could be raised to 16% as an outside amount. He thought a rigid line should be drawn at 16 %, and he believed that if that were done there would be very little difficulty in reducing the quantity of water in commercial butters to that amount. Some people could, no doubt, be found who would excuse even 35 or 40 per cent.; and an inspector from the Cork butter market recently stated on oath that the proportion of water in the butter depended on the atmospheric conditions at the time the cows were milked. Similarly, Dr. Bell had recently stated that he did not see his way to regard butter as adulterated if it did not contain more water than had been known to be left in it, when not manufactured for sale. This seemed to him to be practically making an incompetent dairy-maid the referee under the Food Act.

Mr. Hehner asked Mr. Richmond for particulars with regard to solids-not-fat. The average had been given, but he (Mr. Hehner) would like to have the extremes, and to know how many abnormally low results had been recorded, if any. He thoroughly agreed with Mr. Allen that 15 per cent. was a reasonable and liberal figure to allow for water in butter. It was, he believed, a fact, that in Denmark many samples had been recently met with giving upwards of 15 per cent. As Public Analysts, they were likely to meet with the contention that, on the authority of Professor Stein, butter contained very frequently more than 15 per cent. of water. It must be borne in mind that such samples should not be compared with the samples submitted to Public Analysts, especially in England. The butter lost a notable amount of water in travelling. The limit of 15 per cent. was calculated on trade samples of butter, and not on butters that came directly out of any dairy All sellers of butter knew that almost every keg of butter lost a certain percentage in weight from exudation of water; and most butter merchants did not pay on the original weight as shipped from abroad, but on the actual weight received by them. It was very important to bear in mind that samples coming straight from the dairy would, in the majority of cases, contain more water than butter as sold to the public or submitted to the analyst.

Mr. Woosnam said that his own experience agreed with that of Mr. Richmond with regard to the distribution of the solids in the case of frozen milk. He had found that it was very difficult after thawing to get a fair sample of the milk as originally produced; and he did not think that by passing through a sieve the chance of abnormal samples was appreciably lessened. The solids—which collect on the bottom of the churn, in a layer

when the milk freezes —are with great difficulty re-dissolved, and would, to a large extent, again settle out after passing through the sieve, leaving the supernatant fluid nearly as poor as before. He (Mr. Woosnam) used the Werner-Schmid process a good deal, and the Adams process for the purpose of checking. He did not pipette off his fat solution, but used a small blow-tube with a mouthpiece attached, as in the ordinary wash-bottle.

Mr. Richmond, referring to Mr. Woosnam's remarks, said that the proper course to follow was to strain the ice off and throw it away, it being much safer than thawing. Thawing was certainly the right course to follow if there was only a choice between doing that and sending out first the liquid milk, and then the liquid produced by thawing the ice. With regard to the question asked him by Mr. Hehner, he could not give the exact figures, but he could give an approximation. Out of 13,196 samples received from the country there were 34 yielding less than 8·5 solids-not-fat. 29 came from the farm to which he had already drawn attention. He could not say how many milks were above 9 per cent., but there was only one above 9·5. The abnormal samples were thus very few. As to the question of water in butter, the sample which gave 16·49 per cent. was churned under special conditions, and it was submitted at once to examination. He thought that his results showed that 15 per cent. was certainly a sufficiently high amount to allow. He thought he was perfectly right in maintaining that the Adams process was a correct one. It was the standard process of the Society of Public Analysts, and if they used any other method it was always compared with that. The only claim the Werner-Schmid process had to accuracy was that it gave results which compared well with those of the Adams process. If he wanted to get an idea as to the fat in a sample in a short space of time he used the Leffmann-Beam process, which gave results sufficiently near for such a purpose.

Mr. John Hughes asked Mr. Richmond whether the results of his 23,000 samples were obtained by the Adams or the Werner-Schmid process?

Mr. Richmond replied that the bulk were obtained neither by the Adams nor the Werner-Schmid process, but by the fat-calculation process. The method had been fully explained by Dr. Vieth in former papers; any particular sample was not experimentally exact, but when the average of some thousands was taken, it was not far out.

Mr. Cassal, referring to the sample containing 8 per cent. of solids-not-fat, asked Mr. Richmond if he considered that the term "genuine milk" could be applied to that sample. and whether he would regard it as being of the "nature, substance, and quality" of milk?

Mr. Richmond stated that in his own particular case he had certain standards to work up to. That particular sample did not come up to that standard, therefore he reported it as such, and the necessity for the fine distinction drawn up by Mr. Cassal did not arise.

Mr. Cassal said his object was to elicit from Mr. Richmond whether he would condemn a sample containing 8 per cent. of solids-not-fat; and while he did not doubt that Mr. Richmond would do so, he (Mr. Cassal) thought it advisable that the fact should be definitely stated. To have a standard to work up to was equivalent to having a rigid definition. He again pointed out that any sample containing so low an amount as 8 per cent. of solids-not-fat was not "milk."

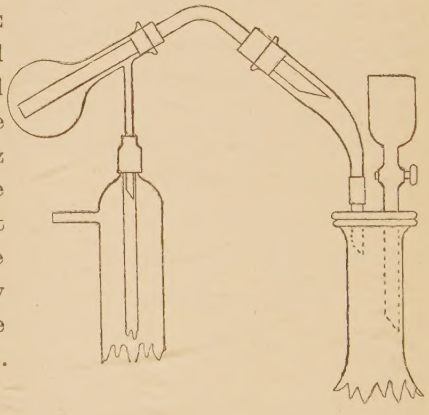
Mr. Allen considered that the analyses of 23,000 samples in the course of a year was a remarkable experience, and that experience was especially valuable when it was added to the 120,000 samples analysed by Dr. Vieth. He had had occasion lately, in Court, in a skimmed milk case, to refer to the valuable statement compiled by Dr. Vieth some time ago; and when the magistrates were told that in these milks the fat ranged from 3·8 to 3·6 per cent., and that the milk in question contained 2·2 per cent., they were much impressed.

In the absence of the author, Dr. Dyer read the following papers :—

THE "STOCK" PROCESS FOR THE DETERMINATION OF NITROGEN. A MODIFIED DISTILLATION APPARATUS.

By W. F. Keating Stock, F.C.S., F.I.C.

The apparatus hitherto used was described in The Analyst for June, 1892 (p. 110.) I have dispensed with one of the two Bunsens there shown, and have given the apparatus the form seen in the accompanying sketch. It consists of a Wurtz flask of not more than 150 c.c. capacity, the tube from which runs into the condenser, and it is kept at boiling point by the ammoniacal steam from the distilling flask. I have found that it entirely prevents the conveyance of sodium hydrate into the distillate, and that it does not retain ammonia. It is a very great gain in simplicity.



THE STOCK METHOD FOR THE RAPID DETERMINATION OF NITROGEN IN ORGANIC BODIES.

A Reply to Mr. W. P. Skertchly by W. F. Keating Stock.

At the May Meeting of this Society I had the honour of introducing to your notice a "New and Rapid Method for the Determination of Nitrogen in Organic Bodies," in a paper which will be found in The Analyst (June, 1892, pp. 109 to 113). During a discussion which followed the reading of my paper, attention was directed to a possible loss of nitrogen by progressive oxidation through ammonia on to nitric acid or even free

nitrogen. At the special meeting held in June, I submitted a reply to this objection, and gave the results of numerous experiments which showed that the feared loss of nitrogen did not occur. This supplementary paper was published in THE ANALYST (Aug., 1892, pp. 152 and 153).

At the meeeting held in October, Mr. W. P. Skertchly read a paper on my process in which he gave a long series of test-analyses which I feel bound to say do him much credit. He proved by these analyses that up to a certain point he could obtain most excellent results over a variety of material, including, like my own test analyses, pure ammonium salts and nitrogenous organic matter. He, however, arrived at a point where, in his hands, the process broke down and failed to yield the whole of the nitrogen he knew to be present in certain samples. THE ANALYST for November contains Mr. Skertchly's paper. Arguing from the results of his experiments, Mr. Skertchly and another member (Mr. Perry Coste) sought to explain by a number of propositions that the failure was directly due to a *loss* of nitrogen in one form or other, and held that until some preventative of the said loss could be discovered, analysts could not rely upon its performance.

I could not see my way to accepting the explanation of Messrs. Skertchly and Perry Coste as being a satisfactory settlement of the difficulty, and I felt diffident about recognising the existence of the several varieties of nitrogen which they suggest might exist in the substances which had proved to be so puzzling. I therefore wrote to Mr. Skertchly and asked him to favour me with duplicates of his samples. This he very courteously and promptly did, and I am now in a position to place my own analyses before you. The samples I received from Mr. Skertchly consisted respectively of Crushed Hoofs and Horns, Fish Guano No. 2, Dried Blood and "Manure." A preliminary inspection revealed the fact that they were by no means so finely divided as I thought desirable for this class of material. The Dried Blood was especially faulty in this respect. I, however, made a trial analysis of this sample in the condition in which it came into my hands. I got 12·90 per cent. of nitrogen as against Mr. Skertchly's 10·82 per cent. I now reduced all the samples by milling, until they passed a 36 sieve, and then determined the nitrogen in each exactly as described in my original communication. The results obtained are given below, side by side with Mr. Skertchly's own figures for his results by my process, and also with those got by himself and Dr. Dyer by the modified Kjeldahl process :—

	THE STOCK PROCESS.		THE MODIFIED KJELDAHL PROCESS.	
Crushed Hoofs and Horns.	*Stock.*	*Skertchly.*	*Skertchly.*	*Dyer.*
Nitrogen, per cent. ...	14·95	13·37	15·34	15·38
„ „ ...	15·09	13·30	—	—

Fish Guano, No. 2.								
Nitrogen, per cent.	...	5·71	...	4·97	...	5·50	...	5·60
,, ,,	...	5·71	...	—	...	—	...	—
Dried Blood.								
Nitrogen, per cent.	...	13·18	...	10·81	...	12·93	...	13·08
,, ,,	...	13·12	...	10·82	...	—	...	—
Manure.								
Nitrogen, per cent.	...	6·24	...	5·56	...	6·16	...	6·09
,, ,,	...	6·27	...	—	...	—	...	—

Here, then, is the true solution of the problem which called forth Mr. Skertchly's very ingenious but groundless "explanations." The simple fact is that, so far from his having *lost* nitrogen, he had, owing in the first place to want of division of his samples, left it unconverted. I say, in the first place, because it is evident on perusal of Mr. Skertchly's paper, that he had taken a good deal of liberty with the mode of procedure laid down by myself. Take, for example, the composition of his oxidant. I prescribe manganese dioxide; Mr. Skertchly uses a substance containing, by his own showing, only 40·86 per cent. of MnO_2. What the remainder was I do not know; but I do know that I can get native manganese dioxide, which shows by analysis from 80 to 97 per cent. of MnO_2.

If I am asked to explain why, in respect of these four samples, my process failed where the modified Kjeldahl succeeded, I answer that Mr. Skertchly did not use *my* process but a bad copy of it, and that he was wrong in the three essential particulars of purity of reagents, condition of sample and relation of quantities. Time is a very important item in any form of Kjeldahl's process. My process is what it professes to be, a *rapid* process, and it also proves to be possessed of a much more important quality, that of accuracy.

Discussion.

Mr. Allen said that a paper of this description was necessarily of a controversial nature, but it was obviously important that any process they employed could be depended on to give correct results. In working the Kjeldahl process he had found it desirable to make two or three determinations and take the highest result. It appeared that some experimenters had abandoned the use of permanganate, and used Gunning's modification, namely, the addition of sulphate of potassium to the acid, with the view of increasing the temperature. Then sometimes, mercury was used and again there were other modifications. Analytical chemists in America had been making a number of experiments with the Kjeldahl process, with and without permanganate, and with and without sulphate of potassium. The results showed that there was no difference in the amount of ammonia obtained. Mr. Stock's method was eminently interesting. It seemed to give results which were fairly in accordance with the ordinary Kjeldahl process. One man did not work under the

same conditions as another, and what would be a most convenient process for one might be very inconvenient for the other.

Mr. W. Pearson Skertchly said that he had followed Mr. Stock's method as nearly as he could. It was only after taking the small quantities of the substance used by Mr Stock that he tried larger amounts, and the results so obtained were more satisfactory than in the former case. Of course, he increased the quantities of sulphuric acid and of manganese dioxide proportionally. All his substances were actually dissolved in the sulphuric acid before the dioxide was added, and he therefore did not think that Mr. Stock had given a good reason for the solution of his (Mr. Skertchly's) difficulty. Mr. Stock in his first paper said that the *manganese dioxide* should be passed through a sieve, having 36 meshes to the linear inch, and in the paper just read he said that the *substance* ought to pass through such a sieve. He did not write his paper with the intention of fault-finding, but merely to show that as he (Mr. Skertchly) had failed with the method, others should not rely on it until it had been proved to give satisfactory results in every case. Personally, he would only be too glad if a quicker process than the Kjeldahl were devised.

Mr. Hehner did not think that Mr. Stock had made out a very strong case for himself. Mr. Stock concluded, from the appearance of the samples given him by Mr. Skertchly, that they were not so finely divided as he deemed desirable, but when he subjected the very first sample, without subdividing it further, to his process, he obtained a result which tallied with that obtained by the Kjeldahl process, but which did not agree with the results obtained by Mr. Skertchly. In spite of this fact, Mr. Stock came to the conclusion that he must more thoroughly subdivide all the other samples. Now, as Mr. Skertchly dissolved the samples first in the sulphuric acid, that disposes entirely of Mr. Stock's explanation of the differences. It was certainly true that Mr. Skertchly had used a bad sample of manganese dioxide, but it appeared to him (Mr. Hehner) that it made no difference whether the manganese was inferior or not, provided it contained sufficient dioxide to oxidise the substance. Mr. Skertchly had added manganese until a green colour was produced, and Mr. Stock considered this to be the criterion of complete oxidation. It was a remarkable fact, that at first Mr. Skertchly obtained good results, and afterwards bad ones; but Mr. Stock had himself, in his earlier experiments, experienced a similar difficulty in obtaining the total nitrogen from potassium ferrocyanide; and though he stated that he had eventually succeeded with that salt, he did not furnish the explanation of his previous failure. When his process was repeated in duplicate it gave agreeing results, but these differed entirely from those obtained by the Kjeldahl process. If precisely the same figure was obtained, he could not imagine how the differences referred to could be ascribed to insufficient subdivision. Mr. Hehner was far from satisfied that even yet Mr. Stock had laid down the exact conditions under which his process would work in every case.

Mr. F. H. Perry Coste concurred with Mr. Hehner in thinking that Mr. Stock had not made out a strong case for himself. As to Mr. Stock's advice that they (Mr. Coste and Mr. Skertchly) should suspend judgment on the matter, that was precisely what he had said. He had spoken very sympathetically of Mr. Stock's process; but it seemed clear that if anyone but Mr. Stock used the process, it was not applicable to ordinary working at present. If other chemists could not obtain the same results, the process required further investigation. He (Mr. Coste) had spoken after deliberate experiment on the behaviour of permanganate. Some time ago, in order to clear up a point in the Kjeldahl process, he had make experiments which proved conclusively that boiling with permanganate of potash did cause a loss of ammonia. He observed that in one determination Mr. Stock had obtained 14·95, as against 15·38 by the Kjeldahl process, a difference of over ·4. It would be interesting to know whether that analysis was made by his own method, or by the *bad copy*, as he called it, which Mr. Skertchly had used. One disadvantage of the Stock process was that the samples had to be finely ground. With regard to Mr. Allen's remarks, experience with the Kjeldahl process showed it to be one of the most accurate in the world; and when one was used to it, it acted perfectly. As to the sulphate of potash in the Gunning process, it undoubtedly was very useful; he had always used it with mercury. The process could be worked without mercury, but it took far longer. With mercury and sulphate of potash three-quarters of an hour was an outside limit even with gelatin.

Mr. E. J. Bevan was very much interested in Mr. Stock's process, partly for the reason that some years ago, in conjunction with Mr. Cross, he had brought out a process for estimating carbon in organic substances by treatment with a mixture of strong sulphuric acid and chromic acid as an oxidizing agent. It had occurred to them that they might be able to estimate the nitrogen in the same way, and they had made some experiments with nitrogenous substances, the results of which were as follows :—

Substance.	Weight taken.	Conditions.	% N. found.	True Percentage. Soda lime.	Kjeldahl.
Gelatin	0·286	CrO_3 added at once	12·92	17·60	17·62
Do.	0·302	Do. do.	12·80	,,	,,
Do.	0·586	Do. previously dissolved, in H_2SO_4	12·80	,,	,,
Do.	0·271	Do. do.	12·95	,,	,,
Do.	0·228	H_2SO_4 only	none	,,	,,
Dried blood	0·273	CrO_3 added at once	9·22	11·64	—

It was somewhat remarkable, that, whilst the percentages obtained were nothing like the actual proportions present, the concordance was very good, although the conditions and the amounts taken varied considerably. In one experiment he got no ammonia at all. In that case he used only sulphuric acid—he did not use any chromic acid —and the result seemed to him to be remarkable, in comparison with the results obtained by the

members of the American Society. In any substance containing nitrogen, when that substance contained a large amount of carbon, there was no doubt that a certain amount of ammonia was formed by the action of sulphuric acid. 0·273 grammes of dried blood gave 9·22 per cent. of nitrogen, the actual amount found by the soda lime process being higher. After seeing an account of Mr. Stock's process in THE ANALYST, he (Mr. Bevan) tried the action of a mixture of sulphuric acid and chromic acid on pure sulphate of ammonia. No ammonia was lost by the action, so the difference had still to be accounted for. He had not examined the product from this reaction, so that he could not say into what form the nitrogen had gone, but it did not go into the form of ammonia. He could not help thinking that it was a pity Mr. Skertchly had not followed the exact details given by Mr. Stock. It was hardly fair to criticise and condemn a process when one worked with four times as much substance as the author suggested. It might possibly be that it was important to take a small quantity Although he agreed with Mr. Hehner that if complete oxidation was obtained, the actual percentage of manganese dioxide in the sample used should not, theoretically, make any difference, still there might be some practical difference; and he thought Mr. Skertchly owed it to himself and to Mr. Stock that he should make the experiments under exactly the same conditions, using the same quantities, and a similar manganese to that used by Mr. Stock.

Mr. John Hughes wished to mention his own experience of Mr. Stock's process. So far as he had gone he had not been able to get the full amount of nitrogen. He quite agreed with what Mr. Hehner had said in reference to the coarseness of the substance. It was a matter which did not require any further argument, because the figures given in the paper obtained equally good results with the coarse, as with a fine substance. He hoped that Mr. Stock would give the members of the Society a little fuller information, and then, perhaps, its excellence would be generally testified to, like the excellence of the ordinary Kjeldahl process. His experience was similar to Mr. Allen's —he could not get the full results which Dr. Dyer and Mr. Coste found no difficulty in obtaining. It might be due to want of skill in manipulation, and he thought the same remark probably applied to Mr. Stock's process. It might eventually prove the better of the two processes.

Mr. Allen agreed with Mr. Bevan that strict adherence to the prescribed method of an author was proper and necessary when making experiments by his process; though he could not see why a process which gave good results on $\frac{1}{1}$ or $\frac{1}{2}$ a gramme should not give the same results on 2 grammes Mr. Bevan's results were highly suggestive. In one of his experiments he had no doubt formed certain amido-acids. Did not his results tend to show that gelatin contained distinct groups, one of which yielded ammonia direct, another by the influence of oxidising agents, while a third was not converted into ammonia; if so, this would seem to throw doubt on the Gunning process. The subject was one which would well repay further attention and experiment.

NOTE ON THE THEORY AND PRACTICE OF THE REICHERT PROCESS.

By H. Droop Richmond.

(*Read at the Meeting, November 2nd*, 1892.)

In a recent number of the *Chemical News* (October 21st), Mr. J. A. Wilson calls into question the accuracy of the Reichert process; in the former paper (*Journ. Soc. Chem. Ind.*, ix., No. 1), he had shown that the rate of distillation of acetic acid was influenced by the salts in the solution distilled, both the nature and the quantity of the salt tending to vary the result; and he argues from the analogy between acetic acid and the acids of butter that the same holds good.

As the Reichert process is now one of our standard methods, it is very important to show that however scientifically correct Mr. Wilson's view may be, in practice the error is quite negligible.

In the *Stazioni Sperimentali Agrarie Italiani*, xxiii., No. 1, I have shown that butyric acid distils according to a parabolic formula; I have pointed out that the distillation is influenced by the amount of acid in solution and by the condensation of the vapour; the rate of distillation is a direct function of the strength of the solution, and the condensation is a direct function of the rate of distillation, or to put it more exactly, of the amount of acid vapour in the total vapour. Were Duclaux's logarithmic formula correct, the condensation would be an inverse function of the amount of acid in the vapour, which is absurd. I have calculated the formula $y = 2{\cdot}22.x - 0{\cdot}0151\ x^2 + 0{\cdot}000031x^2$ (x = quantity distilled from 100 c.c., y = per cent. of butyric acid in the distillate) for the distillation of butyric acid in the Wollny apparatus. No logarithmic formula can be found to express the results with any degree of accuracy.

From this formula I calculate that when 110 c.c. are distilled from 140 c.c., that 96·3 per cent. of the total acid should pass into the distillate; and that when 50 c.c. are distilled from 75 c.c., that 89·1 per cent. should be found in the distillate.

I find, when these proportions are distilled with the addition of the quantities of alkali and acid used in the Reichert-Wollny or the Reichert processes, that I get 93·9 or 89·6 per cent. (uncorrected for impurities in the alkali and acid); and further when 4·4 or 2·2 grams. of well-washed fatty acids are added, that the figures are 97·2 and 89·7 per cent. respectively.

These figures show that for butyric acid the amount of salts in solution in the Reichert process have no practical influence on the results. My results also seemingly controvert Wollny's statement that the fatty acids hold back the volatile acids; they do not really do so. As Wollny's statement is loosely worded, he actually only shows that

the fatty acids, when not completely decomposed (so-called "melted") or when solid, hold back the fatty acids.

As the higher fatty acids distil even more readily than butyric acid, the conclusions drawn from the latter apply with more force to them.

I have experimented with Leffmann and Beam's modification in which glycerine is used, and thereby the boiling point is raised; the excess obtained (for 2·5 grams. 0·07 c.c.) by this modification is much less than the experimental error.

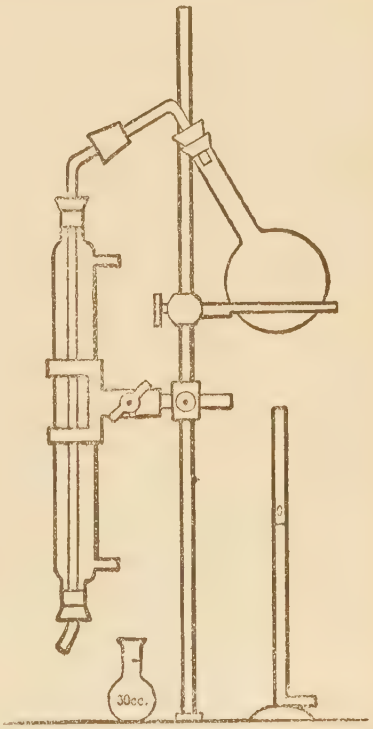
Mansfeld (*Milch-Zeitung.*, 1888, xv., 281), shows that by the use of potash instead of soda no difference in the results is obtained, and this has been confirmed by many other observers.

Mr. Wilson's objections are from a practical point of view groundless, and are based on the assumption that the volatile fatty acids of butter, distilled in a particular way, are influenced by certain external influences to the same degree as acetic acid is when distilled in another way. A study of the laws of the distillation of these acids will show that scientifically he is correct when he alleges errors, but that these errors are so small as to be negligable.

APPARATUS FOR THE ESTIMATION OF FREE AND ALBUMINOID AMMONIA IN WATER ANALYSIS.

By Augustus H. Gill, Ph.D.

The article by Mr. Embrey* upon this same subject, has induced me to describe the apparatus for these determinations in use in the laboratory for water analysis of the Massachusetts Institute of Technology, used by the State Board of Health.

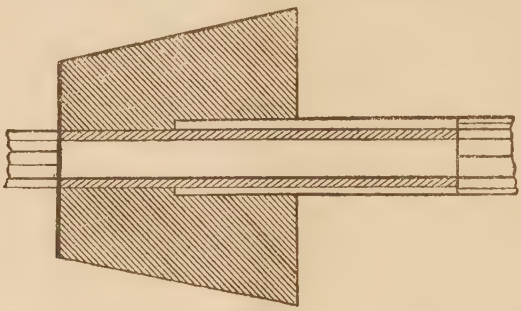


The arrangement will, I think, be evident from the accompanying sketch. The apparatus consists of a nearly spherical flask with square shoulders, of 850—900 c.c. capacity, connected by a bent glass tube and "cork joint" with a vertical block tin condenser. The "cork joint" is here shown in section, and consists of a sound cork fitted at one end with the condenser, and at the other, with the glass tube, indicated by the cross hatching, which enters the former for about an inch-and-a-half, thus avoiding all contact of steam with the cork, and making a

* The Analyst, xvii., 41.

tight and durable connection; the flask is closed with a superfine cork carrying the bent glass tube.

The condenser is the usual glass jacket, with the inside tube of block tin, three ounces to the foot, one-quarter inch internal diameter, twenty-four inches long and bent at the top for the "cork joint," and at the bottom for delivery into the fifty c.c. graduated flasks.† The lamps used are ordinary Bunsen burners lengthened by inserting a piece of pipe between the base and the burner proper. The whole apparatus is supported by the ring and clamp upon a brass rod fixed into the distilling table. This table is eighteen



CORK JOINT.
Full Size.

feet long, two feet wide, and three feet high, with two thirteen-inch sinks near each end; upon it are arranged the two rows of brass rods five inches apart, with eighteen inches between each rod, thus affording space for fourteen sets of apparatus, and making the laboratory equipment capable of handling sixty to seventy samples per day. All the piping is underneath the table, the rubber tubing passing through it, one set of tubing being painted black to distinguish the rows; each condenser and lamp is provided with its own stopcock, or the whole can be turned off together.

For designating the number of the water analyzed, slips of ground glass are placed between the vertical rods.

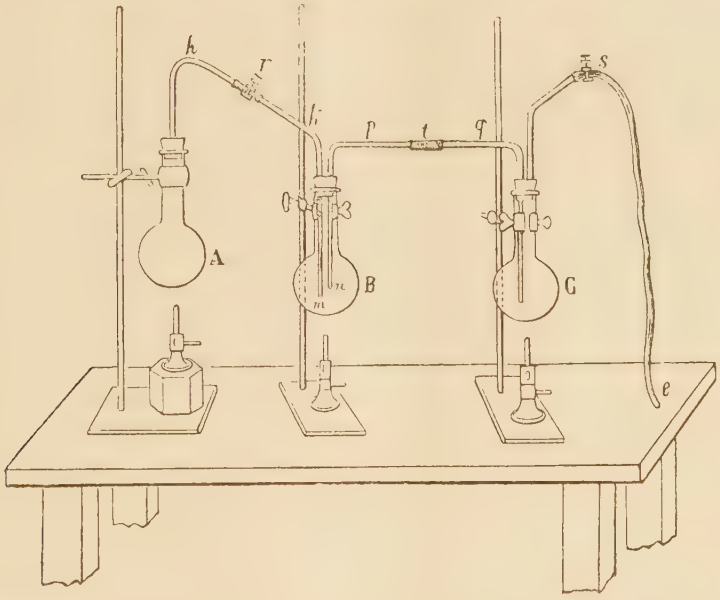
The apparatus is distilled free from ammonia before each determination, and, as we have waters that show neither free nor albuminoid ammonia, it indicates that the working is satisfactory.

The condensation, even in the hottest weather, is absolutely perfect. Spiral condensers of glass and tin have been used, but were too fragile and cumbersome to admit of their continuance.

With this arrangement one person performs fourteen distillations simultaneously in about an hour, the work all being done from one side. The present form has been in use eighteen months, during which time about 5,000 determinations have been made, which is sufficient proof of its practicability.

† Flasks are here used, as the Nessler tubes are inconvenient to manage; those in use hold 50 c.c. are 8 inches long and $\frac{5}{8}$ inch internal diameter, admitting of the reading of 0·000001 gm. of ammonia with absolute certainty.

Observations on the methods in use for the determination of the quantity of gas dissolved in potable waters. G. Musaio. (*Staz. Sper. Ag. Ital.* xxiii. 113.)—In the determination of the amount of gas dissolved in water there are several sources of error, which arise from the entrance of water into the vessel in which the gas is measured; these are the solubility of the gas in the small quantity of water in the tube, and the oxidation on the mercury in the presence of water. Another source of error in the determination of oxygen lies in the absorption by pyrogallate of potash; either the absorption is not complete, or carbonic anhydride is liberated and measured with the unabsorbed gas. To avoid these errors the author proposes a new form of apparatus; this consists of three flasks arranged as shown in fig. 1. The flask A contains the water



under examination, and is closed by an indiarubber cork, through which passes the tube H; the extremity of this tube is drawn out, and is attached to the rest of the apparatus by an indiarubber tube, on which is a screw pinchcock. The flask can thus be filled and placed in position without risk of the water escaping.

The flask B acts as gasholder; it is closed by an indiarubber cork through which pass two tubes, as shown in figure. The obtuse-angled tube is divided in two parts, which are joined by an indiarubber tube just after passing through the cork. When joined the tube passes almost to the bottom of the flask; but if the upper portion is carefully withdrawn, the lower portion drops off, so that any gas in the flask may be completely expelled by the upper portion. The third flask is simply used as a reservoir. The apparatus is used as follows:—The flask B and C are nearly filled with distilled water,

and the corks are simply placed in them so as to allow steam to escape; the water in them is boiled for from 35 to 40 minutes; then the cork of C is inserted, and air is blown in till B is completely filled. The cork of B is next inserted, and this flask is then quite full. The two pinchcocks R and S are closed.

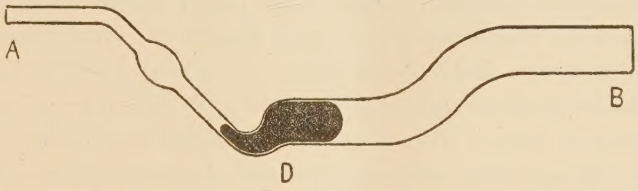
The flask A is filled with the water under examination; the pinchcocks are slightly opened, and air is blown in. When a small stream of hot water issues from the tube the drawn-out end of the tube H is inserted therein, and the apparatus is ready. The gas is lighted under the flask A, and under the flasks B and C when necessary to keep the water in them hot. The gas passes into B, and the displaced water flows into C. After forty minutes to an hour's boiling the pinchcock R is closed and the tube H withdrawn. The tube K is gently raised till the part D M drops off. The tube K can be emptied of water by allowing a flame to play on it for a second or two. A tube of small bore is inserted into the indiarubber tube, and the gas transferred through it into a measuring tube standing over mercury by blowing through the tube E till the water reaches the pinchcock R. No correction need be made for the small quantity of air in the small bored tube.

The analysis is performed by first pushing into the tube a piece of caustic potash, and noting the absorption. This is called carbonic anhydride. The author prefers pyrogallate of potash to cuprous chloride, sodium hydrosulphite, or ferrous sulphate and potash, as an absorbent for oxygen. He pushes up a ball of papier-mâché soaked in pyrogallate of potash, and repeats the treatment until no more is absorbed. He then uses a piece of caustic potash to absorb any carbonic anhydride formed in the reaction between oxygen and the pyrogallate. The residual gas is taken as nitrogen. All gases are reduced to the standard pressure and temperature. The remainder of this paper is devoted to the discussion of a few results obtained with saturated distilled water, and the water of his laboratory. In the latter the results for oxygen and nitrogen were those required by theory for a fully-saturated water, but the carbonic anhydride was notably deficient of the amount required to form bi-carbonates.

H. D. R.

Estimation of Oxygen in Lead. G. Lunge & E. Schmid. (*Zeit. f. Anorg. Chem.* 1892, II., 451 to 460.)—The shortness of re-melted lead has been attributed to the presence of dissolved oxide, and it has been stated that the resistance of lead to sulphuric acid depends on its content of oxygen. The authors therefore deem the determination of oxygen in lead a matter of some little importance. Their method is to pass hydrogen through the molten metal and weigh the water produced. The hydrogen from a Kipp's apparatus is passed through a wash-bottle containing caustic soda, one containing a solution of lead oxide in caustic potash (to absorb H_2S), two

containing silver nitrate solution, and two containing strong sulphuric acid ; the gas then passes through a tube containing platinised asbestos, and heated in a furnace ; this is followed by a wash-bottle of sulphuric acid and two U tubes containing phosphorus pentoxide. The hydrogen thus purified from the last traces of oxygen, is then passed through the molten lead contained in a hard glass tube :—



The hydrogen passes in at the end A, the metal is kept molten at D by a fan-shaped bunsen burner, and the water is caught by a phosphorus pentoxide U tube attached by means of a cork at B. A guard wash bottle of sulphuric acid completes the apparatus. The hydrogen is allowed to pass for two hours to free the apparatus from oxygen, and for another two hours after the platinised asbestos has been heated to redness. The authors made several experiments to prove the purity of the hydrogen at the end of the four hours, and are satisfied that this time is sufficient to ensure the absence of oxygen. The quantity of lead used should be under 30 grammes, and it, of course, must not be melted until the apparatus is absolutely free from oxygen. Half-an-hour is said to be sufficient to deoxidise the lead. The final U tube is then weighed with all the usual precautions.

The following figures may be quoted from the authors' tabulated results :—

	Oxygen Per Cent.
Firsts (Jungfernblei)	0·00237
Do. alloyed with 0·02 per cent. of antimony ...	0·00363
Refined lead, alloyed with 0·02 per cent. of antimomy	0·00250
Do. do. 0·1 „ of copper ...	0·00343
Do. do. 0·2 „ „ ...	0·00566
Do. do. 1·0 „ „ ...	0·03661

Attention is called to the influence of copper in increasing the dissolved oxygen.

A. G. B.

Sulphurous-Acid in Wine. M. Ripper. (*J. Prakt. Chem.* [2], xlvi., 428—473.)—The author has made a searching investigation into the condition in which sulphurous acid exists in wines, and into the methods for its estimation ; his experiments are fully detailed in the paper, which is of considerable interest.

Haas' method (*Berichte*, 1882, xv., 154), for the estimation in question consists in distilling the wine in a suitable apparatus, through which a current of carbonic dioxide is passed, and collecting the distillate in a bulbed U tube containing iodine solution; the sulphurous acid is thus oxidised to sulphuric acid which is weighed as barium sulphate. Desiring to use this as a standard process, the author sought for possible errors in it and alighted upon oxygen and hydrogen sulphide in the carbon dioxide as likely to influence the results; he states that the former gas is always present in carbon dioxide, made from marble and hydrochloric acid, and that the latter may in many cases be detected. These sources of error would tend to neutralise each other had they any real existence; that they have can hardly be allowed when the numbers obtained with carbon dioxide washed by water only, and by potassium permanganate and chromous chloride are compared. A more real source of error was found to exist in the impurity of the barium sulphate, upon which point the author has already written. (THE ANALYST, 1892, 233).

As an analytical process Haas' method is too lengthy and the author modifies it by collecting the distillate in caustic potash, acidifying this solution and titrating it with N/50 iodine solution. The details are as follows :—50 c.c. of the wine are pipetted into a distillation apparatus through which a current of hydrogen is passing. 5 c.c. of sulphuric acid (1 : 3) are added and the flask is heated in a glycerine bath at 80°—85° for three-quarters of an hour, the current of hydrogen being maintained the while. The sulphurous acid is absorbed by 20 c.c. of N-potash in the receiver, and after the time stated is liberated by the addition of 10 c.c. of sulphuric acid (1 : 3) and titrated with N/50 iodine solution, starch being used as an indicator. A number of experiments are quoted to show that no other easily oxidisable, volatile constituent of the wine passes over with the sulphurous acid Experiments were then made to ascertain whether the direct titration of the sulphurous acid in a white wine be possible. With regard to red wines it may be said that they seldom contain sulphurous acid, inasmuch as their colour would be damaged by sulphuring; should an estimation be necessary it must be made by the distillation process. The fact that the iodine consumed by an acidified white wine is far below that necessary to oxidise the sulphurous acid present, as estimated by the distillation method soon brought the author to the conclusion that the sulphurous acid in wine is not all present as free acid or as alkaline sulphite. After many experiments it was found that aldehyde sulphite is the other form in which the sulphurous acid exists in the wine; this is not immediately oxidisable by iodine in presence of sulphuric acid, but as might be expected, is split up by the acid during distillation: it is, however, noteworthy, that the aldehyde and sulphurous acid in the distillate will again combine after a short time if no substance be present to remove one or the other. Very brief contact with caustic potash will decompose the aldehyde sulphite, and this opens the door for the determination of the *total* sulphurous acid in wine by direct titration. To this end 50 c.c. of the wine are pipetted into a 200 c.c. flask containing 25 c.c. of normal potash, the nose of the pipette being inserted into the liquid; after some 10—15 minutes 10 c.c. of sulphuric acid

(1 : 3) are added, and titration with N/50 iodine solution effected, starch being added as indicator. The *free* sulphurous acid is then determined by pipetting 50 c.c. of the wine into a flask which has had a stream of carbon dioxide passed through it, adding 5 c.c. of sulphuric acid (1 : 3), and titrating at once with the N/50 iodine until the blue colour is permanent for a short time, after which the tannins, etc., in the wine begin to beoxidised. The difference between the total and the "free" sulphurous acid (*i.e.*, that existing as such and as alkaline sulphites), is termed "aldehyde" sulphurous acid by the author. By adding sulphurous acid to unsulphured wines, he showed that when the wine is kept the free sulphurous acid decreases and the aldehyde sulphurous acid increases, while the total sulphurous acid also decreases.

The author gives no summary as to the quantity of sulphurous acid in wines, but from the figures he quotes it would seem to average a little over 0·01 gram. per 100 c.c of the Rhine wines which he analysed. A. G. B.

REVIEWS.

THE PRINCIPLES OF THEORETICAL CHEMISTRY. By IRA REMSEN. Fourth Edition (London: Bailliére, Tindall & Cox; 7s. 6d.)

The object of this work, as stated in the preface, is "to help students to get clear ideas in regard to the foundations of chemistry;" and, keeping this in view, the author has succeeded in producing a book which is not only extremely useful for the purpose for which it was originally intended but will be read with pleasure and profit by many who have long passed the period of studentship. The matter is placed before the reader in such a charming manner that he is unconsciously carried away with the subject, and experiences no tedium in the perusal of the work. Whilst in strict keeping with the present state of chemical theory, the author most sedulously distinguishes between the proven and the unproven, and carefully warns the reader against the prevalent error of over-theorising. The following statement is particularly apropos, and is one which many will cordially endorse;—"It cannot be denied thst we are now in a period of chemistry which may fairly be called one of *formula worship*. More value is sometimes attached to a formula than to that which it is intended to represent. In consequence of this, it has happened that a large number of chemists have regarded the determination of a formula for a compound as the great object to be accomplished, and they have forgotten that what we ought to know, and what is of vastly greater importance to the science, is the chemical conduct of the compound." The fact that, within a comparatively short time, three editions of the work have been exhausted and a fourth demanded, shows that it must have attained a considerable amount of popularity, Since the appearance of the last American edition, Italian and German translations have appeared. W. J. S.

UNTERSUCHUNGEN AUS DER PRAXIS DER GÄRUNGSINDUSTRIE. VOL. II.

By DR. EMIL CHR. HANSEN. (R. Oldenburg, Munich.)

This is a continuation of the account of Dr. Hansen's investigations in that difficult portion of the brewer's art which relates to the physiological processes concerned in fermentation, a branch of science in which this highly gifted author has so eminently distinguished himself.

The first chapter treats on the analysis of air and water for micro-organisms from a brewer's point of view. In this the author shows the futility of examining water by the method of Koch as used for hygienic purposes, since, as he points out, by far the larger portion of the organisms which develop on the gelatin plate are absolutely innocuous to either wort or beer. He prefers, therefore, to experiment directly on sterilised wort or beer.

In the second chapter, Pasteur's method of purifying yeast by cultivation in a wort acidified with tartaric acid is criticised at considerable length, and it is shown that, though this process readily frees yeast from bacteria, it favours the development of wild yeasts and suppresses the cultivated ones. Thus the gain on the one hand is counterbalanced by the loss on the other, and consequently it is impossible to obtain a pure yeast in this manner.

Chapter III. is devoted to an investigation of the diseases of beer caused by the Saccharomycetes. In it we find the interesting fact mentioned that Scheele was the first to apply practically the principle of sterilisation by heat. This he did in 1782, the substance operated on being vinegar, and the method he used does not differ materially from that in use at the present day. The author considers that it is in the large coolers that the wort principally receives its infection from the wild yeasts present in the air. He shows that the breeding ground of these organisms is the surface of fruits and berries in summer, and that in winter they lie dormant in the ground.

The fourth and last chapter gives a *resumé* of the extension of the author's system of pure yeast culture in recent years, and this has been very considerable. It is gradually coming into use in this country; and we learn that yeast grown from a single cell has been in use for some time past in one of our largest London breweries, where it has yielded the most satisfactory results.

A further development of the author's views has been applied to wine making. It is found that the flavour of the wine of a particular district depends on the variety of yeast found on the grapes of that district. Consequently, by fermenting the grape juice of any district with yeast from another district, the flavour of the wine is that of the district from whence the yeast was procured.

This process of selection is even applied to bacteria, pure cultures being used for souring the cream in cheese-making. The same principle is also applied to the fermentation of tobacco, for by selecting the bacteria different flavours can be produced at will.

W. J. S.